Explaining human origins

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an archaeological dialogue with Wiktor Stoczkowski

On a drizzly Thursday morning in the autumn of 1994, from the offices of the Leiden Ice Age Foragers Research Group, we have a beautiful view of that key site for the history of natural history in the Netherlands: the Leiden University Botanical Garden, dating back to the late 16th century. In this appropriate setting, I am talking to Wiktor Stoczkowski about his recent contribution to the history of the natural history of man, *Anthropologie naïve, anthropologie savante: De l'origine de l'homme, de l'imagination et des idées reçues*. At an appropriate moment, too, for on this very Thursday morning, the discovery in Ethiopia of one of the earliest hominids yet known, *Australopithecus ramidus* (White, Suwa & Asfaw 1994), is front-page news all over the world. One of the burning questions in commentaries and discussions: did it walk erect? The subject of the lectures Dr. Stoczkowski had just given at Leiden University: the history of the notion of bipedality.

Wiktor Stoczkowski (1959) was educated as a prehistorian and anthropologist at Warsaw University. Subsequently, as a research fellow at the Polish National Museum of Archaeology, he worked on the epipaleolithic and mesolithic cultures of northern Poland as well as on the ethnohistory of Native American cultures in the Canadian subarctic. In 1986, he went to Paris to join Jean-Claude Gardin's research group on theoretical archaeology at the CNRS. For six years, he worked on the structure and backgrounds of explanations of the origin of man in palaeoanthropology, reporting on the results of this project in a doctoral thesis, a number of articles, and in the aforementioned monograph, which is based on that doctoral thesis. Since 1992, Dr. Stoczkowski has been lecturing at the Université de Lille II, where he is a member of a multi-disciplinary research group on the anthropology of knowledge.

Anthopologie naïve, anthropologie savante analyses the principal reconstructions of human evolution since the early 19th century, from J.B. de Lamarck, Ch. Darwin and F. Engels through K. Oakley and C.S. Coon up to recent interpretations such as those of E.O. Wilson, C.O. Lovejoy, G. Isaac and A. Zihlman. Stoczkowski shows how and explains why the enormous growth, in recent decades, of our knowledge of early hominids in their natural surroundings has not fundamentally altered our basic explanatory schemes. He traces these schemes – such as, for instance, that of the transition from a paradisiacal Golden Age to an era of desperate, or heroic, struggle with hostile nature – through the 19th and the 18th centuries all the way back to antiquity, arguing that, basically, the whole development of views of human origins has taken place within the narrow constraints of one single 'generative matrix', which, until now, has never fundamentally been challenged.

Dr. Stoczkowski, what's the origin of your interest in the cultural and historical backgrounds of interpretations of human origins? How, as a practising prehistoric archaeologist, did you get into the history of ideas and the history of science?

You urge me to narrate, to reconstruct narratively my own history, my own intellectual development and quest. But perhaps a biography, any biography, my biography, is basically chaotic, without much clear sense, governed by chance and coincidence! What's the origin of my recent preoccupations? The notion of 'origin' as a critical, pivotal point, the original event at the basis of all the rest, as in psychoanalysis and in myth – psychoanalysis is another myth, of course – is precisely one of the fictions which are analysed and criticized in the approach I take. But, joking aside now, I think a certain disappointment with my activities as a rather traditional prehistorian, working on epipaleolithic and mesolithic sites of northern Poland, has been important in this respect. It had to do with the poverty of the answers I could formulate to concrete, basic research questions, which, however, still fascinate me. Out of this frustration, I turned to ethnoarchaeology. This enabled me to formulate much richer answers. The funny thing, though, was that at the same time they were much less well-founded, and it struck me that, ironically, the less well-founded they were, the more eagerly they were adopted by my colleagues. So I started wondering why certain interpretations, which theoretically as well as empirically seemed to be quite weak and contestable, should be allowed so much credit by scientists. At this point, I started delving into the history and the methodology of the discipline, into the sediments, or fossils, of the imaginary, so to speak, next to, and, consequently, instead of, the real sediments.

Who were your maîtres à penser?

I don't like this, of course very French, expression. In my opinion, thinking loses its value at the very moment it submits to a master. The very existence of 'masters' is linked with such gestures of submission, which enhance the persistence of simplistic stereotyped notions launched by such authorities. Of course I have sometimes been seduced by certain ideas of certain authors, but never by some conception or thought in its totality. I am fond of reading great thinkers and scientists, but primarily in search of quite specific ideas and views which are interesting heuristically for what I am working on at that moment. Usually that is not at all what the author had in mind.

Most of your readers, however, will probably be struck by similarities between your method and that of Claude Lévi-Strauss. What's your position vis-à-vis him, and structuralism in general?

I do not consider myself a structuralist, although I am a definite admirer of Lévi-Strauss' accomplishment as an ethnologist. I find his methods for analysing representations and some notions he proposes – like that of *bricolage*, that of structural transformation, that of permutation of preconceived elements, and so forth – very useful, and perhaps more interesting than the substance of some of his general views of man and culture. For me, structuralism is a method of analysis, more than an explanatory theory of man and culture. I gratefuly apply some of Levi-Strauss' methods to a field which he himself has never applied them to, namely scientific theories. Specifically, I found structural transformations in the history of western views of human origins, which time and again bring into play the same, invariable basic ideas in varying combinations. Lévi-Strauss analysed non-western myths in this manner, synchron-

ically, because of the small time-depth of his ethnographical data. I work *dia*chronically on *Western* historical materials. My most recent work, in progress, along these lines is on the very rich, but almost completely unknown history of the notion of bipedality, from Plato and Aristotle through Cuvier and Daubenton up to present-day controversies.

One might still say you qualify for the label 'structuralist', if only by adopting the basic idea of 'long term structures' (structures de longue durée) *in the history of thought, which, as you show, function as a 'generative matrix' for interpretations of human origins.*

Perhaps, but let's not forget that I also use other methods, such as 'logistic' analysis of scientific argumentation as developed by Jean-Claude Gardin, and similar methods applied in the field of artificial intelligence. Few structuralists have worked on scientific texts, and none of them have used such methods. Also, I built upon a rich French tradition of studies on the history of science, especially on the history of natural history. Once again: I do not care for such labels! Structuralism is just a tool-box for me, and I pick whatever appears to be handy for solving my problems. One does not choose a fork to cut with, nor a knife to fell a tree. I would, for instance, use Derrida's poststructuralist approach as well, if only I had come across only one single useful element – which is not the case.

In her controversial Narratives of human evolution, *Misia Landau compares a number of classic texts on human evolution to the hero tale in folklore, using a structuralist – or, more precisely, 'morphological' – approach inspired by Vladimir Propp's studies of Russian folk tales (Landau 1991). Peter Bowler (1991) has criticized her for neglecting the role of specific, changing historical contexts, and dealing with scenarios of human evolution as if they were eternal archetypes, or timeless pieces of literature. Do you agree?*

Bowler, who is not the only one to have made this comment, is right when he remarks that Misia Landau does not pay much attention to the historical contexts – race, class, gender, imperialism, and so on – in which the theories she studies arose. But, on the other hand, she has never denied the existence, or even the importance, of those contexts either. She simply focuses on another aspect than most other historians of science favour, Peter Bowler included. I think, however, that her approach is seriously flawed in another respect. I happen to read Russian, and to be familiar with the Russian folk tales Propp worked on, as a result of my training in ethnology, which included much folklore. In my opinion, and indeed that of most experts in this particular field, Propp's readings of these tales are not very accurate (cf. Stoczkowski 1992). Therefore the similarities Misia Landau claims between these tales and palaeoanthropological scenarios of human evolution are simply not there, although there are similarities between the ways Landau as well as Propp deform the contents of the texts they work on. Also, she might have taken account of more recent developments in the field of structuralist methods to her benefit.

You do not give that much attention to specific historical an cultural circumstances yourself either.

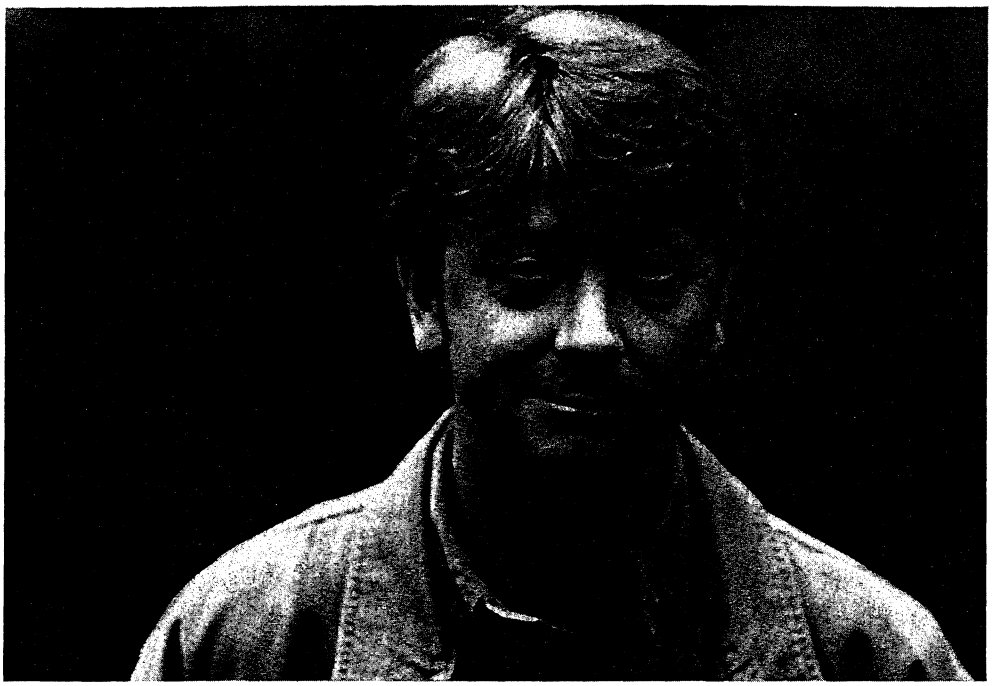

Figure 1. Wiktor Stoczkowski, Leiden, autumn 1994. Photograph Wil Roebroeks.

That is correct. But then, I did not set out to *explain* the history of a number of anthropological ideas, but to *describe and trace* them. One of the first and in the end, as it turned out, most important results, much to my own surprise, was the very persistent character of certain ideas, whose influence lasted for centuries – whatever the historical circumstances. So these *structures de longue durée*, as I call them, playing with a term coined by Braudel, far from being some kind of structuralist preconceived idea of mine, are something I came across when systematically analysing the logical structure of twenty-four texts on human origins representing several centuries of changing – as well as *un*changing – views. In most cases, I was able to trace back to antiquity the origins of such interpretative schemes as bipedality as a sign of human uniqueness and dignity, the sexual division of labour, primeval promiscuity, or certain ideas on the origins of language. In other words, the role of such ideas in, for instance, 19th-century palaeoanthropology cannot be explained *exhaustively* in terms of the specific scientific, institutional, ideological, historical contexts of the period. Of course such contexts have been important, but I chose to focus upon those hitherto neglected deep roots and long-term continuities. I like to think of my own modest contribution as complementary to current approaches which are more sensitive to immediate, short-term historical backgrounds, such as those of George Stocking, Pietro Corsi, Adrian Desmond and Peter Bowler in the English-speaking world, or, in France, those of Claude Blanckaert, Goulven Laurent and others.

Complementary, then, also in the paradoxical sense that most historians of science, in the French tradition as well as in the English-speaking world, tend to stress discontinuities – 'epistemic ruptures', 'paradigm shifts' – over continuities!

Figure 2. 'Primeval man', from P. Boitard, Etudes antédiluviennes: Paris avant les hommes, *Paris 1861, and analysed by Stoczkowski (1994). A hairy, club-wielding creature standing at the beginning of the long progress toward civilization. Its anatomical features are influenced by a tradition of publications on the great apes, especially Edward Tyson's anatomy of a chimpanzee published in 1699. Certain features of early representations of apes and prehistoric man can be traced back to the Plinian Races from antiquity and the Wild Man of the Woods from medieval folklore and art.*

Indeed, but again: it is a matter of research focus. It depends very much upon the sort of problems you are interested in. The history of science is usually perceived in terms of innovation, change, criticizing and breaking with older ideas. The image – compelling by its sheer simplicity, seductive, but too facile – of sciences that change the world, and change themselves in the process, while the world changes, has directed the attention of historians primarily towards those changes, and led them to neglect continuities. The role of age-old, stereotypical interpretative schemes is a feature the importance of which tended to be underestimated.

You use the past tense?

"Tended to be underestimated", as this turns out to have been changing in recent decades, as witnessed, for instance, by Pietro Corsi's work on the emergence of Lamarckism. Ideally, one should try to keep a balance, or, more precisely, to reconstruct the balance, and the interactions, between continuities and discontinuities, but normally one stresses either the one or – usually – the other. Often, I would like to add, I see myself forced to stress continuities somewhat over discontinuities, for instance in my recent work on the history of ideas on bipedalism. In my view, there was no sharp epistemic rupture either here, or in many other fields, in the transition from folk or 'naive' anthropology to scientific anthropology.

Don't you underestimate the role of empirical constraints in theoretical developments in prehistoric archaeology, palaeoanthropology, and related disciplines?

The *anthropologie savante* is supposed to be distinguished from the common-sense *anthropologie naïve* from which it arises – and which, I would add, it feeds upon – precisely by its endeavour to account for empirical findings. But in the materials I analyse I find no sharp break in this respect. The palaeoanthropologists whose texts I analysed in great detail – a task that took a full year – did not find the empirical data all that important when articulating their basic, most ambitious views. Their conceptions usually did not originate from ancient soils, but from ancient texts. An interesting case in point here are the earlier interpretations of *Ramapithecus*, the explicitness and comprehensiveness of which was inversely proportional to the number of available fossils. And then, of course, philosophers of science have made us aware, if you will forgive me this cliche, that all observations are full of theoretical assumptions – there are no pure data, uncontaminated by expectations and interpretations.

Let's take another example: the 'deconstruction' of Glynn Isaac's ideas on base camps and food-sharing.

That's a complex issue, but one of the things I find interesting here is the fact that Isaac's theory of early hominid behaviour exactly fits my model, for he interpreted most of his archaeological data in terms of these preconceived ideas. That he subsequently gave up this theory and urged his students and collaborators to tackle the sites from as many interpretative angles as possible says much for the quality of his scientific effort. Regrettably, not all prehistorians are able to do away with a favourite hypothesis in such frank a manner.

But, on the other hand, many philosophers of science now agree that it makes sense for scientific paradigms to stick tenaciously to certain pet assumptions for at least some time, as it permits them to become fully articulated, to be confronted with empirical data systematically, and, ultimately, makes them perish or triumph in rivalry with other theories.

I agree; one does not need to be a Lakatosian to agree with that. The problem with what I call 'anthropologie naïve', however, is that it saddles us with virtually unverifiable ideas which last for centuries in nearly unchanged form, without any serious confrontation with data. These preconceived ideas are as abstract as they are powerful, and permeate the whole of the discipline of palaeoanthropology, friend and foe alike, without much confrontation taking place between theory and data or between alternative theories. In that sense I would call those preconceived ideas 'dogmatic' in a less positive sense than Lakatos' methodologically sound immunization of the 'hard core' of a research programme. Even in recent decades one has tinkered with notions like bipedality, man the hunter, man the toolmaker, the conquest of the savannah, etc., the one leading to the other, or the reverse, continually changing the causal links – the specific constellation of what leads to, and thus explains, what. It's a kaleidoscopic palaeoanthropology indeed, to borrow a metaphor from Lévi-Strauss' *La pensée sauvage*. In the process, these prefabricated elements are never really addressed and questioned themselves, as I show in my book.

So you conceive of these traditional interpretative schemes as a hindrance rather than as a heuristically invaluable source of inspiration? On page 137 of your book you equate the 'naïve anthropology' they issue from with 'a trap', and on page 205 you call it 'trivial'.

I tend to stress their negative impact, because I see how they have impoverished rather than enriched our scientific images of the past, and continue to do so. They present themselves as self-evident, while in fact they are one-sided, simplistic, and questionable. They thoughtlessly recycle the same ideas time and again. On the other hand, there have been a few empirically fruitful ideas, now and then.

Yet the narrative imagination should be given credit for its competence with respect to creatively making sense of and incorporating new data. Isn't there a considerable interpretative power here, and thus, basically, a certain type of rationality?

Man is a rational and creative being, and of course this human condition has its bearing upon every human activity, including narration. But I think Lévi-Strauss is right when he points out that change in mythic or narrative activities is a *bricolage*, a matter of transformations upon a limited number of traditionally given elements. That is precisely what one sees in discussions on the origins of language or the enlargement of the brain: a tinkering with prefabricated elements, without much care for empirical validation. One tries to understand, for sure, but in the process order and sense are constructed without strict constraints being imposed by the scarce – too scarce – empirical data. I would like to add, however, that the notion of narrativity, as used in current discussions, is too vague a notion to be very useful.

What about the refined, highly technical structuralist narratology of A.J. Greimas and the so-called Paris School, which builds upon, among others, Propp and Lévi-Strauss, with its notions of protagonist, quest, contract, conflict, value system, and so on?

Greimas' approach certainly has a considerable heuristic value, as shown, by the way, by some of your own work on European notions of savageness and primitivity. Apart from offering refined conceptual tools for tackling specific research questions, it also suggests the inadequacy of the narrative scenarios which have traditionally been used for reconstructing the processes palaeoanthropologists deal with. By their very nature, those narratives force crude binary oppositions upon the always ambiguous, and, as such, permissive data, as well as teleologies – such as that of the laborious, but ultimately inevitable heroic conquest of civilization. Upon close scrutiny, however, nothing of that kind is to be found on the sites or among the fossils. It is my definite feeling that narratives not so much help us as *prevent* us from perceiving prehistoric worlds in their full complexity and richness!

Let's take another example: the current debate on 'ancient' versus 'modern' hominids.

A salient example indeed of the pervasiveness of binary oppositions: 'archaic' versus 'modern', associated with 'animal' versus 'human'. Here we are dealing with one of the most sensitive, and indeed heavily tabooed, conceptual distinctions we, humans, make – the human-animal boundary. Here our own identity is at stake, perhaps even more than in palaeoanthropology in general. Such an interpretative framework suggests, and permits us to see, discontinuities, but at the risk of overstressing them over similarities and continuity, at the risk of constructing too sharp boundaries, at the risk of oversimplifying what in reality are very complex processes and changes.

One of the central topics in this 'archaics' versus 'moderns' debate is why, how and, particularly, when ...

... language appeared? Right! Were the Neanderthals fully capable of speech? Some hold they were; others, like Chris Stringer and Clive Gamble in their recent book on the Neanderthals (Stringer and Gamble 1993), hold they were not. In some of my darker moods, I tend to advise researchers in this evasive field to choose another profession, such as construction worker or wine-grower. There is so much unfruitful speculation on the origins of language, and so few data to go on! As early as 1860, the *Société Linguistique de Paris* decided to refuse from then on any new treatises on that subject matter, because of its speculative character. On the other hand, recent research is quite promising. Particularly exciting are the new perspectives on early hominid language which have arisen from recent research on the linguistic competence of chimpanzee and bonobo. Here too the importance of the history of the discipline is evident: current research has much to learn about theoretical possibilities as well as theoretical dead ends from the vast 18th-century discussions on the linguistic capabilities of the apes. I agree with Robert Wokler, who made this point convincingly here in Leiden, one year ago, at the Pithecanthropus Centennial Congress.

What's your view on the distinction between the 'context of discovery' and the 'context of justification', and on what level would you situate your analysis of long-term structures?

Another perverse question! Do you really think such sharp distinctions are possible? It would seem to me to be just one more of those binary oppositions which are so pervasive in myth, the history of ideas and the history of science, and you know what I think of these: at best, they are working tools, simplifying the world very much, without necessarily being false, though.

An example which is often used to illustrate the difference between the 'context of discovery' and the 'context of justification' is the discovery of the chemical structure of benzene by Kékulé von Stradonitz in 1865...

... who is supposed to have dreamt about a serpent biting its own tail, or, in another version, seeing a flame in his fireplace doing the same thing. Another origin myth, available in different guises! It's an oversimplifying explanation. Thousands of people will have seen such images without ever connecting this to the chemical structure of benzene. The image alone is not sufficient; Kékulé did make the association because he was at that time working on this particular chemical problem. There is so complex and continuous an interaction between the two levels that it is virtually impossible to tell them sharply apart. Such a distinction is usually constructed from, if not forced upon the data *ex post facto*, with the benefit of hindsight.

So what about your own work?

The foregoing having been stipulated, one might see my work as focusing mainly on what is usually called the context of discovery: what's the background, what are the discursive and conceptual roots of theoretical views. The authors whose texts I unravel, including those from recent decades, usually do not justify their theoretical views at all, because they take them to be self-evident. *Why* certain basic ideas could be taken to be self-evident is one of the baffling things my book tries to deal with. Among other things, it has to do with their level of abstraction, which is high. There is a lack of empirical evidence on this level. In the discipline I studied, palaeoanthropology, there is an enormous growth of empirical knowledge, but at the same time, alongside this fast growing body of data and virtually unhampered by it, I came across the persistence of those traditional interpretative frameworks time and again.

My last question, Wiktor. You reconstruct a sort of deep grammar or syntax governing the thought of palaeoanthropologists and archaeologists. What's its use for practising scientists?

If we don't reflect upon the boundaries imposed upon us by those, so to speak, 'grammatical' rules, of which we normally are largely unconscious, we risk never being able really to transgress them. Truly innovative concepts may not only require putting that grammar to novel use, but also reflecting and improving upon that grammar itself. In theoretical archaeology, over the last thirty or so years, we have developed archaeological approaches from

philosophical and ideological preconceptions such as positivism, relativism, phenomenology, hermeneutics. Too little attention has been given to systematic studies of the concrete products of archaeologists: site reports, regional monographs, regional or chronological syntheses. I am convinced such studies would show up the huge impact of our most fundamental, more or less naïve, more or less spontaneous preconceptions of the quintessence of man, culture, history. Archaeological and palaeontological remains do not give forth their information spontaneously. They just answer the questions we ask them. The quality of the answer depends upon the quality of the question, even where there is abundant data. We can enrich our dialogue with the data by becoming aware of the poorness of our questions.

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